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ABSTRACT

The range of answers to the question, "What is Science?" has literally reached beyond the capability of any one person to adequately understand and summarize. This paper discusses the concept of the so-called "value-free" science, and contrasts this traditional mode of inquiry with the new modes of thought espoused by advocates of feminism and poststructuralism. It is concluded that the current conflict and disagreement over the nature of science is a positive situation and one which brings together many differing perspectives to the benefit of research. (PR)

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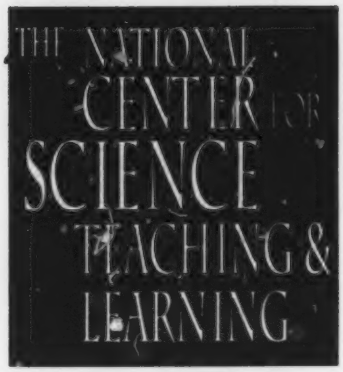

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Methodological Implications of Feminist and Poststructuralist Views of Science

By Jim Schuerich

In this thought-provoking paper, NCSTL researcher Jim Shuerich discusses the concept of so-called "value-free" science, and contrasts this traditional mode of inquiry with the new modes of thought espoused by advocates of feminism and poststructuralism. In his "story," as he calls it, Shuerich states that the current conflict and disagreement over the nature of science is a positive situation, one which brings together many differeing perspectives to the benefit of research.

THE QUESTION—WHAT IS SCIENCE?—GOES TO THE HEART OF THE SCIENTIFIC ENDEAVOR. While the answer to this question may seem obvious, there currently is considerable ferment and conflict over the different answers that have been proposed throughout the sciences (Lather, 1991). In fact, the range of answers has literally reached beyond the capability of any one person to adequately understand and summarize. There is the famous work of Thomas Kuhn and of all of his detractors and advocates. There is the sociological work of Michael Lynch, Steven Woolgar, and Barry Barnes in England. There is the work of such poststructuralists as Foucault, Derrida, and Deleuze in France. There are critical theorist views of science like that of Habermas. There is the scientific realism of Bhaskar and Outhwaite. There are feminist efforts to define science, including the work of Harding, Hartsock, and Keller. There are hermeneutical views of science like that of Gadamer and, before him, Dilthey. There are views of science that are based on the intersection of two perspectives, views like the poststructuralist feminism of Lather and Flax. The list goes on and on.

The point I am making by referring to the different and competing views of science is that I do not claim that the perspective I support in this paper is the only one. Nor do I want the perspective I support to become the dominant one. Indeed, I prefer the current situation in which there is conflict and disagreement. I want, thus, to emphasize at the outset that my view, my story, is only one of many and, further, that our understanding of science is better served by open encounters with many different stories of what science is. My support of such plurality does not mean, however, that I will not try to argue my view persuasively. I will, like anyone presenting a paper, use cunning strategies of logic and rhetoric to make my story sound like a story you might want to embrace. I believe in the story I am telling in this paper; I be-

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lieve that it is worth your consideration and, even, your agreement.

The topic of my paper, or my story, is "The Methodological Implications of Feminist and Poststructuralist Views of Science." I do not approach these views as a critic; I approach them primarily as an advocate. While it might not seem questionable that I as a male am an advocate of poststructuralism, it may seem unusual that I am an advocate for feminist views of science. I can only say that I find feminist writings on science and its methods to be exciting, challenging, and insightful. Moreover, I would hope that everyone, women and men, would increasingly become aware of the incredible outpouring of scholarly work occurring within feminism. I do not mean to imply, though, that I am a feminist. Indeed, in my opinion, while a man can support feminism, he cannot be a feminist. Some leading feminists, such as Harding (1987) would disagree with this, at least in the sense that, according to Harding, men can "do feminist social science" (p. 11). She says that her "own preference is to argue that the designation 'feminist' can apply to men who satisfy whatever standards women must satisfy to earn the label" (p. 12). I simply think that feminists are doing very important work in terms of defining what science is and what it might be, and I want that work to inform my work. Consequently, when I consider the question of "What is Science?", my considerations inevitably include feminist answers to this question. It would be impossible to summarize the broad range of feminist thinking, with its many internal disagreements, within the limitations of this paper, let alone also summarize the broad range of poststructuralist work, including many of its internal disagreements. Instead what I am going to do in this paper is, first, discuss one important issue which many feminists raise about the practice of science and, then, I am going to discuss what the methodological implications of that feminist issue are. I am not going to discuss any similar poststructuralist issue, because of time limitations, but I have included a kind of poststructuralist subtext within my methodological recommendations.

One Feminist Issue

ONE OF THE MAIN POINTS ASSERTED BY FEMINISTS IS that feminist scientists, working openly out of a biased perspective—feminism, have produced better scientific work than that of scientists working out of a supposedly value-neutral perspective (Alcoff, 1987; Bleier, 1986; Harding,1991; Keller, 1985; Millman & Kanter, 1987; Rose, 1986; among many others). For instance, Sandra Harding (1991), one of the best known and most widely respected of the feminist epistemologists, in her latest book, *Whose Science? Whose Knowledge?*, says that in science there is a "greater objectivity attainable by starting [scientific] research from women's lives" (p. 135). This point of view seems to contradict one of the central premises of the conventional idea of science, i.e. that good scientific work depends on the exclusion of bias, the exclusion of values, the exclusion of politics, the exclusion of the sociological. But feminists are claiming the exact opposite: they are claiming that their feminist values, their feminist politics, their sociological positionality as women scientists produces "better" scientific results than conventional science. As Linda Alcoff says, "The very existence of a category called feminist social science creates a philosophical problem" (p. 85).

The most widely cited example of this in feminism is Carol Gilligan's (1982) research on the moral development of women. Prior to Gilligan's work, Kohlberg, a male scientist, had developed a widely respected scheme for the stages of moral development that was assumed to apply to all people. According to Gilligan (1987), "Kohlberg claims universality for his stage sequence..." (p. 68) of moral development. But Kohlberg's conclusions, according to Gilligan (1982, 1987), were based on men's lives and men's orientations to morality. Gilligan (1982) found, in contrast, that when women were studied in the context of women's orientation to morality, the universality of Kohlberg's stage sequence was undermined. From the feminist perspective, Kohlberg's value-free science had produced value-loaded results.

Many similar examples of feminist revisions of apparently value-free science exist throughout the research literature. The most prominent example in my field, educational administration, has been the feminist work of Charol Shakeshaft. Decades of supposedly value-free scientific work had been done on administration in general and on educational administration in specific. The result of this value-free research has been various theories of administration that the researchers and theorists claimed was universal. In 1987 Charol published a book entitled *Women in Educational Administration*. In this work she showed that some of the most prominent and widely accepted theories of administration had been drawn overwhelmingly from research on male administrators and had utilized, almost exclusively, male orientations to administration. In contrast she demonstrated that research on women administrators which utilized women's orientations to administration indicated that the prior research was anything but universal. Once again, feminists could claim with ample justification that a feminist biased science had produced better science than a value-free science.

If these two examples are correct, and if there are many more that show the same result, we appear to have what Kuhn (1970) would call an anomaly in our theory of what science is. It was the persistence of a replicable anomaly, as Kuhn so ably demonstrated, that undermined "normal" science and initiated a period of "revolutionary" science in which a new paradigm and a new language superseded the prior "normal" one. It would appear that this would be the case in this instance also. If feminists can show, as I think they have, that their "bias" can often produce better science than a value-free approach, the conventional view of science is, at least, questionable and, at most, displaced by a new view of science.

"Once again, feminists could claim with ample justification that a feminist biased science had produced better science than a value-free science."

There are, however, possible arguments against the latter conclusion. One possibility is that feminists have simply uncovered and corrected some bad science. In this view, the claim of universality for theories of moral development or theories of educational administration was incorrect. It is not that the science was bad; the claim of universality was incorrectly asserted. While the conclusions of conventional science were accurate for male moral development and for male administrators, they should not have been extended to women.

This defense of the conventional view of science leaves unaddressed the issues of why a supposedly value-free position yielded bad science and why a biased position like feminism produced a correction of bad science. Feminists would probably answer the first question in terms of the social dominance of women by men and the second in terms of emancipatory politics, but what is more important to this discussion is the implications of these two questions—why did a value-free science produce bad science and why did a feminist biased science produce good science—in reference to defining or redefining science.

The feminist answers to the two questions, whether you agree with those answers, raise the point that science is a practice that occurs in specific social and historical contexts, that science is practiced by socially positioned individuals, by socially gendered individuals. The scientific ideal has been, however, that scientists could develop rigorous procedures that, if carefully

followed, would protect against social biases. As Harding (1991) says, "value-free, impartial, dispassionate research is supposed to direct the identification of all social values and their elimination from the results of research..." (p. 143). The traditional answer of theorists of science to this particular problem has been to divide the scientific process into the context of discovery and the context of justification.

The context of discovery is that part of the scientific process in which hypotheses are developed; the context of justification covers the testing of the hypotheses. It is only in the latter area, the context of justification, that conventional science guarantees no bias if rigorous methodology is followed. There are no guarantees about the context of discovery and, thus, no guarantees against biases in hypotheses. As Harding (1987) says,

> There is no logic for...'contexts of discovery,'.... Instead, it is in the 'context of justification,' where hypotheses are tested, that we should seek the 'logic of scientific inquiry.' It is in this testing process that we should look for science's distinctive virtues (for its 'method'). (p. 7)

If this arrangement is accepted, the problem with the work of Kohlberg or the researchers in educational administration is contained within the context of discovery where no scientific guarantees against bias exist.

The speciousness of this division of the scientific process is obvious, at least in the social sciences. If the scientific method provides no guarantees about biases in hypotheses, those biases can be carried through not only to research results but also to educational practices. This is clear in Shakeshaft's research on educational administration. The prior, gender-biased work, which was based on "value-free, impartial, dispassionate research," resulted in assumptions that kept women from entering and succeeding in educational administration. In the social sciences with their connection to professional practices we cannot afford the luxury of such biases whether they occur in the context of discovery or not. Consequently, for the social sciences, at least, the feminist challenge is very much to the point.

Unfortunately, the point is not as simple as it might first seem. Subsequent feminist research has criticized Gilligan's research and other feminist research as being itself biased in favor of white middle class women (see, for example, Dill, 1987, or Huggins, 1991). This more recent feminist work suggests that lower class women and women of color differ in significant ways from white middle class women. Thus, Gilligan, who critiqued Kohlberg for universalizing, stands critiqued herself for universalizing the moral development of white middle class women to all women. Fee (1986) concludes that "there is no feminist position that can transcend the boundaries of class and race" (p. 54).

The point, however, of this latter challenge to Gilligan is the same point as Gilligan's original challenge to Kohlberg. Biases arising out of our social positionality and the socialization that results from that positionality significantly influence our practices of science. But these biases are not merely negative; they are both positive and negative. On the negative side, the biases of conventional science excluded women and women's perspectives; the biases of white middle class feminists excluded the perspectives of lower class women and women of color. On the positive side, the bias out of which feminism operates helped expose the limits of Kohlberg's work; the bias out of which feminists of color worked helped expose the limits of Gilligan's work. If bias has this doubled character, what does that mean about how we might want to change our definitions of what science is and, thus, change our methodological practices in the social sciences?

Methodological Implications

ONE RESULT OF THIS FEMINIST CHALLENGE IS THAT in the social sciences we cannot ignore the context of discovery either as an area of critique and examination or as an area for the application of method. Those of us in professional practice cannot ignore the deleterious effects that biased research conclusions can have on our practices, even if the reliability and validity of such research is guaranteed by the methodological practices applicable to the context of verification. As we have had verification methods, we must have discovery methods. The kind of methods I have in mind, however, are very different from such verification methods as reliability or validity.

To develop such methods we must keep in mind the doubled character of bias as both negative and positive. We must also remain aware that there will always be new biases that we either do not know at present or that we know but have suppressed. We can, in other words, never be free from the important caution that we are never free of bias. Poststructuralists have suggested, rightly I think, that all points of view suppress other points of view; to develop a coherent point of view means excluding that which is incoherent to that view but coherent to another view (Flax, 1990). Foucault (1977) even suggests that there is a certain violence involved in the development and maintenance of any one point of view.

Gadamer (1975) in his seminal work, *Truth and Method*, suggests that bias is the necessary condition of the social and historical positionality of humans, that it cannot be escaped by the scientific method or any other method. He further argues that this does not have to be taken negatively, that living through our biases is a constructive way to engage our lives. He does not mean, however, that we should ignore biases that are inequitable. He means that we can use the bias of our social positionality in a positive way, exactly as the feminists have done.

The question is how can we build positive methods out of the biases of our social positionality that will constrain or illuminate biases that hurt, biases that reproduce inequity, biases that are violent. What I would suggest is both simple and complex. One of the rhetorical conventions of traditional scientific writing is that the author does not exist in the text. Part of the "value-free, impartial, dispassionate" approach to science, which is drawn from the exclusive focus on the context of verification, is that the scientist as a social person does not exist within the scientific work. The feminist examples discussed above suggest the opposite.

> "...we openly put the researcher back into the scientific process, where she or he has always been anyway."

When the context of discovery is included within our methodology interests, the specific author of a research text and the author's social positionality is very much in the text. I do not mean to imply, though, that the biography of a particular individual or the sociology of the same individual is as simple as the person's statement of her or his gender, race, and class. There are substantial individual differences among the members of any combination of gender, race, and class. There are also substantial differences among and within various combinations of gender, race, and class.[1] Speaking from a feminist poststructuralist viewpoint, Patti Lather (1991) claims that "The subject [an individual] is neither unified nor fixed. We occupy conflicting subject positions..." (p. 118).

Nonetheless, the socialization that attends a person's social positionality is very powerful. Women, on average, do receive a very different socialization than men do. As Carol Warren (1988), a feminist anthropologist, says, "Being a man or woman is at the core of our social lives and of our inner selves" (p. 10). In addition, African Americans, on average, receive a very different socialization from that of whites or Hispanics or Native Americans or Asians. Lower class individuals receive, on average, a different socialization from that of middle class or upper class individuals. Hence, I am suggesting that, on one hand, we do not want to oversimplify the immense complexity in the intersection of individual biographies and the social positionality of those individuals, nor, on the other hand, do we want to ignore the powerful effects of socialization.

What I am suggesting is that we incorporate within our research process and within our articles about such research a significant degree of reflectivity about our individual biographies and about our social positionality. More simply, we openly put the researcher back into the scientific process, where she or he has always been anyway. This researcher, however, is different from the conventional science researcher that was hidden in the process. This researcher is reconstituted or reconstructed as a socially reflective researcher, as a researcher that is persistently self-conscious about her or his personal biography and social positionality and the positive and negative effects those might have on the research process and the published report of the research. Again, though, this reflectivity cannot be reduced to simply reporting one's gender, race, and class within a research text. The kind of social self-consciousness that I am proposing is more profound, more complicated, more suffused throughout the research process and text. No matter how far we might proceed in this direction, nonetheless, we also need to be aware that self-consciousness is always limited: there is always much about ourselves and our socialization of which we are not aware.

The reflectivity that I am recommending is, of course, not original. Feminists, like most social groups that have been marginalized, have recognized all along the importance of being self-aware about one's social positionality. For example, Sandra Harding made this same recommendation in 1987:

> The best feminist analysis...insists that the inquirer her/himself be placed in the same critical plane as the overt subject matter, thereby recovering the entire research process for scrutiny in the results of the research. That is, the class, race, culture, and gender assumptions, beliefs, and behaviors of the researcher her/himself must be placed within the frame of the picture that she/he attempts to paint. This does not mean that the first half of a research report should engage in soul searching (though a little soul searching by researchers now and then can't be all bad!). Instead, ...we are often explicitly told by the researcher what her/his gender, race, class, culture is, and sometimes how she/he suspects this has shaped the research project—though we are free to arrive at contrary hypotheses about the influence of the researcher's presence on her/his analysis. Thus the researcher appears to us not an invisible, anonymous voice of authority, but as a real, historical individual with concrete, specific desires and interests (p. 9).

Patti Lather near the beginning of her book *Getting Smart: Feminist Research and Pedagogy With/In the Postmodern* (1991) indicates that she is "a first-world woman—white, middle-class, North American, heterosexual," but she also discusses in a footnote to this description and in a later chapter both the importance and the limitations of such descriptions of one's social posi-

tionality. In addition, Lather is also a good example of a feminist social scientist who is well aware of the doubled character of point of view as both positive and negative.

Social scientists of color have long had a similar awareness. For example, Ladner (1987), as "a Black social scientist" (p. 76), openly brings a "Black perspective" (p. 74) to her study of low-income African American women. Her report of this research is thoroughly laced with reflectivity about her own personal and social biography so that, as Harding says (1987), the researcher is brought inside the same "critical plane" as the subjects of the research and is, thus, available to our scrutiny. In "Field Research in Minority Communities: Ethical, Methodological, and Political Observations by an Insider" Maxine Baca Zinn (1979), as a Chicana and a social scientist, argues that "insider" researchers have important advantages for research on minority communities. Anthropologists have also picked up on the use of self-reflectivity in their work. Vincent Crapanzano's *Tuhami: Portrait of a Moroccan* (1980) is a good example of this new type of work in anthropology.

IN SUMMARY, I HAVE ARGUED THAT FEMINIST WORK is a valuable contribution to our efforts to construct or reconstruct what science is and, subsequently, to change some of our methods in science. Feminists have shown in different instances that their feminist biased approach to science has produced better science than apparently value-free approaches to science. This raises a fundamental challenge to our conventional views of science. Such traditional views of science have typically solved this type of challenge by dividing the scientific process into two parts: the context of discovery and the context of verification. It is only the latter part that the scientific method warrants the lack of bias. This solution, however, is woefully inadequate for social scientists whose research results are often used as the basis for professional practice.

The feminist challenge to conventional views of science introduces the importance of addressing biases of social positionality that are not addressed in the context of verification. Consequently, methods that are applicable to the context of discovery need to be devised that make scientists more self-reflective about the sociological biases arising out of their particular social positionality. I have followed a common feminist suggestion that the researcher "be placed in the same critical plane as the overt subject matter, thereby recovering the entire research process for scrutiny in the results of the research" (Harding, 1987, p. 9). The social scientist re-enters the research process and the research text as a socially self-reflective individual. Due to the limits of self-consciousness, however, we cannot become complacent that this is a simple process. It calls, instead, for a continual and profound self-questioning that is thoroughly embedded throughout both the process and the textual results of scientific research. I have also pointed out that many examples of this approach are already available within feminist research, race/culture-oriented research, and anthropological research. ■

Notes

[1]See, for example, Millman and Kanter's (1987) grief note about men and women and the fact that the powerful and powerless inhabit different social worlds (pp. 33-34).

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